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When the liquid is cold add 50 grains of carbolic acid. Let the liquid stand five or six days, and then filter. It is claimed that by means of this fluid the colors of larvæ can be preserved perfectly, even when exposed to a strong light.

ZOOLOGY.

Turning Hydra inside out; a Correction.—Will you allow me to correct a slight mistake that has crept into the pages of the AMERICAN NATURALIST? On page 387 of the April number it is stated that I have succeeded in turning Hydra inside out. The credit of having accomplished the feat should be given to Mr. C. Ishikawa and Mr. M. Kikuchi, both my assistants at the time. They devised simple but ingenious instruments for the purpose, and made the task of turning Hydra inside out a comparatively easy one, as I can testify from personal trial. Mr. Ishikawa made at the time many observations on the behavior of Hydra thus turned inside out. So far as I remember, Hydra, under these circumstances, generally quickly gained the right side out again, and if forcibly prevented from doing so by having a bristle put through its body, it still made frantic and almost laughable efforts to right itself. No definite results were reached, so far as I can recollect, in regard to the change of functions in the layers of the body,—at any rate something interrupted Mr. Ishikawa's observations before any definite conclusions were obtained, and that is the reason that nothing has been published until now. I hope that Mr. Ishikawa will take up the investigation again some time, and throw more light upon this interesting subject.—*K. Mitsikuri*, *Imperial University*, *Tokio*, *Japan*, June 2, 1887.

A Remarkable Case of Phosphorescence in an Earth-Worm.—On the night of June 20, 1887, when returning from placing some Phengodes females where they might attract the males, I saw in the path before me a small phosphorescent light. As I approached it became larger; now an inch, and the next moment two inches long. I was on the point of declaring I had found another Phengodes when suddenly it became three, four, and six inches long, now darting in this direction and then in that.

Thinking there might be an insect capable of running and giving a phosphorescent light to objects in its path in such a way as to cause them to be illuminated for a short time, I struck a match so that I might see to capture it. As the light of the match overcame the phosphorescence and lighted up the objects on the ground, the only insect I saw was a Myriapod scampering away under the leaves. All this took place in less time than it takes to tell it. Thinking the insect was hidden under the leaves, when the light of the match was extinguished I began to brush them away with my hand. The phosphorescence appeared again, and the light advanced as rapidly as I moved my hand. I thought certainly I was in pursuit of the prize, and in my anxiety to capture it I would catch up a whole handful of earth. But I always noticed that I left the phosphorescent light on the ground. The insect eluded me, I thought. After pursuing the phantom in vain for some time, I stopped to ponder. After the habit of a puzzled man in an inquiring mood, I brushed my foot across the earth before me, when lo! each little pebble and pellet of earth bristled with a phosphorescent light. For a moment I seemed to lose confidence in the fact that all such phenomena are due to natural causes. The air was sparkling with fire-flies, and the earth at my feet was bristling with phosphorescence. Had time sped backward twenty centuries, and were the vestal fires lighting around me? Or was some great calamity impending? I seemed to hear these words repeated from the sky: "*Nate dea, quo fata trahunt retrahuntque, sequamur; Quidquid erit, superanda omnis fortuna ferendo est.*" But in a moment more I had brushed away these delightful superstitions, and went to work to find the *cause.*

Taking a quantity of the earth, I went to my study. First I took several pebbles into a dark room and rubbed them in my hands; no light appeared. Then I took some pellets of earth and treated them in like manner; result the same. Surely, I thought, the phantom lies in these decaying leaves, but disappointment was also the result of the third investigation. I returned to my package of earth, and saw a lone earth-worm writhing in pain. "I will try him," I thought. I took it into the dark room. It showed no sign of phosphorescence. I rolled it once across my hand. The *phantom* became *real.* Not only was the whole body of the earth-worm luminous, but the entire palm of my hand was aglow with phosphorescence. I made several trips to the place, and secured other worms which exhibited the same peculiarity.

Probably the Myriapod which I saw had pounced upon one of these worms for a meal. It became phosphorescent, which may have frightened the Myriapod, and it ran, dragging its prey, and the worm's body touching the ground illuminated it, thus attracting my attention. When the earth is disturbed, the friction of the body against the particles gives phosphorescence to them. The earth-worm is probably a species of Lumbricedæ.—*Geo. F. Atkinson, University of North Carolina, Chapel Hill, N. C.,* July 7, 1887.

Dwight on the Teleology of Bone-Structure.—Professor Dwight, of Harvard, thus concludes a paper on the cancellated structure of the bones of Vertebrata, in the Memoirs of the Boston Society of Natural History: "It appears, therefore, evident that so far from the actual structure of the bone being the only possible one, it in many cases presents useless features, and that certainly there must be some determining factor besides teleology. Is it impossible to hold that the vertebræ of an alligator, for instance, could not answer their purpose equally well if the internal structure were more on the plan of mammalian vertebræ? Are there, then, not peculiarities of race that, in some cases at least, do not answer any definite purpose? It seems to me that there can be no reasonable doubt on the matter. It may be urged that we lack the knowledge to decide whether any given organism considered as a whole is or is not as good as its nature admits. It may be urged that an essential element of perfection is the due proportion of parts and faculties, and that it is impossible for us to say that the greater development in some particular direction would be for the welfare of the individual. I have no desire to dispute the great truth underlying these propositions, but in view of rudimentary organs alone, it would appear that, at least in some subordinate details, useless structures occur, and we have no means of deciding what limits to assign to the action of the causes producing them. It is certain that these appearances are not due to chance; there must be some determining cause modifying the structure in this direction. It is customary now to quote rudimentary organs and anatomical anomalies as evidences of descent, but it seems to me very improperly, occurring as many of them do quite out of the line of inheritance. This criticism applies with greatest force to anomalies, but rudimentary organs seem phenomena of the same general class. What principle, then, shall account for them? We must turn to homologies. We see within vertebrates a general plan; and, though great modifications occur, the plan persists. No liberties, so to speak, are taken with it beyond a certain point. There are never, for instance, more than two eyes or one mouth or four [*sic*] pairs of limbs. Is there reason to be sure that none of these, or analogous modifications, might not be for the benefit of the individual? Yet I think every student of natural history would look on the suggestion that they will never occur as chimerical.

"The principle of homology has a restraining influence on variations of structure, both in quasi-accidental instances, as in anomalies, and in transformation, of species (if that occurs), by restricting changes within the limits of the general plan.

"To condense further these deductions, it appears that the internal structure of any particular bone may show evidence of three factors: first, that of teleology; second, that of homology; third, that of correlation to the structure of other bones of the same animal. The relative prominence of these factors varies greatly. For example, in the humerus of the whale the first is of little moment, and so is the third in the heel-bone of the seal. Thus, while we find provision for the fitness of the part, we find also sometimes apparently useless structures, sometimes apparently evidences of degeneration, but throughout are more or less distinct marks of harmony with other parts, and of homology with other forms. How has this been accomplished? Clearly the crude notion that accidental, purposeless, external forces should be sufficient to change by slow degrees one such organism into another of a different species is untenable. The doctrine of chances alone shows it to be impossible. There is, moreover, the unanswerable argument of the inevitable uselessness of incipient structures. Where we see the need, we see the structure to meet it already perfect. We see also the combination of homology with teleology. Whatever, therefore, the share of evolution may be in the production of species, it is not one of chance. The changes must be for the most part comparatively sudden, and therefore due to an implanted, internal force acting in predetermined directions. On the theory of external accidental forces, the preservation of homology is incomprehensible. The action, however, of this internal force is, no doubt, modified by accidental secondary causes, which may produce degenerative as well as progressive changes."

Note on the Food of Amblystoma mexicanum.—In a recent note in *Science*[1] Dr. R. W. Shufeldt has placed on record some interesting "Notes on the Diet of Amblystomas," in which are given his experiments with meats of one and another sort in connection with captive specimens of the common *A. mavortium* of the Rocky Mountain regions. It has occurred to me that the following brief notes, incidentally made during the past winter, and based upon the contents of the alimentary canal of several alcoholic specimens of the common Mexican *Amblystoma* from Lake Texcoco, may prove interesting as connected with the ordinary food-habits of the free form. During the examination of some thirty specimens, conducted for anatomic purposes, it was found that twenty-two individuals contained from one to five specimens of *Cambarus montezumæ* Saussure,[2] some of which were still entire. Five specimens contained numerous examples of *Physa mexicana*, intermingled with which were many fragments of insects. In nearly all the specimens occurred many *Gammari* sp. indt. One individual contained a *Pisidium*, closely allied to *P. abditum*, associated with a half-digested minnow too much decomposed to admit of generic determination; the fish, when entire, must have been three inches in length. No plant-remains were found, save a few small roots, which were invariably associated with the *Cambari*, and doubtless swallowed with them. More or less mud was found in every stomach examined, the greatest quantity in those stomachs containing the *Physæ*. Attached to the fauces of several individuals were found sometimes as many as a half-dozen specimens of a nematode worm belonging to *Strongylus*. The stomach of one individual was completely devoid of food-remains of any sort, but was completely filled with clusters of this nematode hanging from its walls.

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[1] Vol. ix., No. 216, p. 298, March 25, 1887.

[2] I am indebted to the courtesy of Prof. Walter Faxon, of Cambridge, for the determination of this form.

It would seem, therefore, that in a free state *Amblystoma* is carnivorously omnivorous.—*R. Ellsworth Call.*

The Turkey-Skull.—Dr. R. W. Shufeldt has recently published the results of his comparisons of the skulls of wild and domesticated turkeys, undertaken with a view of noticing the changes brought about by domestication. His results in brief are:

1. In the wild bird the nasals fuse with the frontals, but in the domestic bird they are separated by a persistent suture.
2. In the wild turkey the cranio-frontal region is wider and more concave than in the tame variety.
3. The parietal prominences are more evident, and the median longitudinal from these to the occipital ridge is shorter in the wild than in tame birds.
4. The occipital area in the wild turkey is cordate, the apex upwards; in the tame turkey it is roughly semicircular.
5. The intraorbital septum is entire in the wild bird, perforate in the tame.
6. The pterygoids are longer and more slender in wild than in tame varieties.
7. The skull is denser and thicker, as well as smoother, in the tame than in the wild bird.

These points and three others mentioned by Dr. Shufeldt hold good for the majority of specimens, but in a large series of skulls examples may be picked out which are intermediate in position. The characters are all comparative, and exceptions are numerous. The studies are not of a diagnostic but of a comparative character, and form an interesting addition to our knowledge of the changes produced by domestication in a comparatively short time.

Anatomy of the Auks.—Dr. Shufeldt, in studying the visceral anatomy of specimens of *Brachyrhamphus marmoratus* and *Synthliborhamphus antiquus*, notes that these two forms, closely allied in the systems, differ greatly in their visceral anatomy. In the ancient murrelet the lower part of the larynx is covered in front by a large mass of fat, lacking in the marbled murrelet. The lateral and sterno-tracheal muscles are the only tracheal muscles present, and these agree quite closely, except that the latter are given off much higher in the marbled than in the ancient murrelet. The differences in the hearts are quite marked, but may result from the action of the preserving-fluid, as this organ is described as smaller and more pointed in one than in the other. The livers differ considerably, being longer and larger in the ancient than in the marbled species. Considerable differences are also noted in the full bladders. Dr. Shufeldt calls attention to the fact that well-preserved spirit specimens of many Arctic birds are still desiderata in our museums, and requests others who may have the opportunity to make comparative studies on other auks.

Domestication of Wild Fowl.—My success so far this year has been better than last, although far short of what I expected. There seems to be no available information on this subject, at least none that I have had access to, and I have for the past few years been learning from failures. Last year I hatched four Chinese mandarin ducks and they lived a week. Their deaths may be attributed to close confinement, and, perhaps, to an artificial diet of corn-meal, boiled potato, and boiled egg.

This year I turned out in a swampy piece of land, with a large spring pond in it, seventeen pairs of wood-ducks, ten odd drakes, and one pair of pintails. They were confined on about an acre of ground by a wire netting three feet high.

All winter these birds had been together and had paired off. They were confined in a small enclosure near one of the windows of the fish hatchery, and were at all times in sight, and it was noticed that the females made all the advances through February and March. Therefore the extra drakes were thought to be harmless. About the first of April they were turned out in the swamp, and furious fighting was noticed among the males, and by the middle of April three of the ducks were seen with their scalps torn off. It was also noticed that the males gave them no rest, and then, perhaps too late, the extra males were removed. Nests were provided for them by placing boxes about two feet from the ground in the alders, and in one of these a duck laid ten eggs; in another three were laid, and two eggs were found upon the ground. These were placed under hens, and from them I have three young, now, July 8, two weeks old, which are strong and vigorous, but refuse all food that we offer, and spend their time in catching flies. They roam out alone, but come up to the building with the old hen.

Back of my house, around a fountain, we made an enclosure which provided a good run of grass, and there I placed one pair of mandarins and four pairs of wood-ducks. The wood-ducks did not lay, but the mandarin laid twelve eggs; nine of these were placed under hens, and five hatched. Three eggs were left with the mandarin, and after setting a week she abandoned them. Of the five mandarins two were killed under the hen. One was a perfect albino, but lived only four days; the remaining two are vigorous.

In a ravine which has a spring pool in it I placed two pairs of blue-wing teal. One of these birds was the mother of the rest, and was raised by a man in Iowa, but no eggs have been laid by them.

My pintail duck made her nest on the ground in a clump of ferns, and set on seven eggs. The day that she was due to hatch she disappeared, and no trace of her, the young, or even the egg-shells, remain. We think she escaped somewhere, perhaps over the netting, for I have seen a wood-duck, although pinioned, go over a wire netting, by means of feet and wings, when first placed in strange quarters. They would go up with a sort of running fly and fall on the other side.

It is possible that, had the wood-ducks been turned out in pairs, and no odd drakes been placed with them, the result would have been better. There is a great deal of natural food in their enclosure, and they have all the corn, wheat, and oats that they can eat; they prefer corn to anything else and eat that first; next they take wheat, and only eat the oats when nothing else is left.

Next year I hope to try again under slightly different circumstances, such as proper mating and different food, and in the mean time I will be glad to have the experience of others in this line.—*Fred. Mather, Cold Spring Harbor, N. Y.*

Zoological News.—ECHINODERMS.—In the *Zoologische Anzeiger* for May 31 H. Ludwig points out that the so-called parasite of Pterotrachea described by Barrois under the name *Trichœlina paradoxa* is in reality the pedicellarium of some Echinoid (probably *Sphærechinus granulatus*) which had become detached. It was regarded by Barrois as a problematical form with Echinoderm affinities.

The readers of Romanes's work upon starfishes will be interested in Dr. Preyer's experiments on the motions of these forms as detailed in a late number of the *Mittheilungen* of the Naples Zoological Station.

WORMS.—Mr. A. G. Bourne, now in India, writes to *Nature* of June 9 calling attention to the sensory powers possessed by the leeches. After alluding to Whitman's observations on the land-leeches of Japan, he records that he took a stone from the bed of a brook with his hand, and almost immediately several leeches came swimming to the spot. Thinking that smell or taste might have had something to do with attracting them, he returned the next day and stirred the water with a stick, but without producing any result. He then washed his hands in the water, taking care not to disturb the ooze on the bottom, when the leeches reappeared and swam about as if they realized the presence of a living being.

Ludwig Plate describes, in the seventh volume of the Naples *Mittheilungen*, some new ectoparasitic Rotifers from the Gulf of Naples. They belong to the family Seisonidæ, and, like all the other members of the group, occur attached to Nebalia. The forms described, four in number, belong to the new genus Paraseison, which is characterized by an absence of intestine, and has the wheel either as in Seison, or reduced to tactile bristles, or, lastly, entirely absent. The other genera of the family—of which a synopsis is given—are Seison of Grube, with two species, and Saccobdella of Van Beneden and Hess, with a single species.

MOLLUSCS.—Prominent among the papers in the twenty-sixth volume of the *Journal de Conchyliologie*, just completed, may be mentioned an account of the Molluscan fauna of the valleys of the Tage and the Sado, Portugal, by Nobre; descriptions of new species of Molluscs from Annam, by Wattebled; from Cambodia and Tonkin, by Morelet; and from China, by Hende.

EMBRYOLOGY.[1]

A Theory of the Origin of Placental Types, and on certain Vestigiary Structures in the Placentæ of the Mouse, Rat, and Field-Mouse.—The discoidal placenta has been supposed by Balfour to have been derived from an earlier and less specialized diffuse type. He, however, gave no connected, and, as it seems to the present writer, no plausible view as to the causes leading to the gradual restriction of the placental area. Although I have already given partial expression elsewhere to the following views, I am now able to state them with greater clearness, owing to the fact that the placentation of the common mouse has very unexpectedly afforded some remarkable evidence in proof of the doctrine of the mechanical genesis of placental types first suggested by me.

The view which the writer holds is essentially as follows: (1) The primitive form of the placenta approximated that of the so-called diffuse type. (2) The primitive type of uterus was duplex or bipartite, and its cornua were therefore tubular. (3) Development in one of the cornua of such an uterus, as the blastosphere enlarged, must have caused the latter to touch the uterine mucosa around a zone embracing its equator, just as a sphere thrust into a flexible tube would be in annular contact with the inside of the latter. (4) If the blastosphere became elongated or fusiform within the tubular uterine cornua, as in Carnivora and Ungulata, a still greater portion of the outer surface of the blastosphere would be brought into contact with the mucous membrane of the tubular uterus, and the conditions for

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[1] Edited by JOHN A. RYDER, Ph.D., Biological Department, University of Pennsylvania, Philadelphia.